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ANALYSIS

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HYPOTHETICALS

By DAVID PEARS

THIS article treats of hypotheticals which say something about the non-linguistic world, something which does not owe its truth solely to convention. They will be represented by " If this is a raven it is black " (η), and its parent general hypothetical " If anything is a raven it is black " (θ) : both spoken by someone who was prepared to face a counter-example. These two hypotheticals are simple and stark : but in this investigation bones must be shown on an X-ray film before flesh is shown in technicolour.

Treated first as truth-functional statements they will present difficulties. One of these difficulties will be shown to be largely illusory : another to be inevitable from any human point of view ; and the rest to be the inevitable result of the logician's professional neglect of descriptive words. In the second part of this paper it will be shown that none of these difficulties can be evaded by calling hypotheticals ' rules ' ; though this name is in some ways more appropriate. Thirdly and finally something will be said about the way hypotheticals are embedded in different locutions.

First, then, taken as truth-functional statements they present five connected difficulties. General hypotheticals (open ones) are not completely verifiable (*a*). What verification they do get is sometimes peculiar (*b*). The verification of singular counterfactual hypotheticals is always peculiar (*c*). '$\supset$' involves two symmetrical paradoxes (*d*). And singular counterfactual hypotheticals do not seem to be deducible from their parent general hypotheticals (*e*). *a* is at least the inevitable result of human myopia. It will be shown that *b*, *c* and *d* are inevitable if a constant meaning is sought for ' If—then ' : so that their remedy is not to seek a stronger constant meaning than '$\supset$', but to recognize the factors which logicians' blinkers hide from view, factors which vary too widely to permit any single criterion of reasonable hypotheticals. And finally that *e* is largely illusory.

If hypotheticals are taken as truth-functional statements, the full fourfold matrix must be used. For, as will appear later, the view that a hypothetical is applicable only where its antecedent is true, though it emphasizes what is important, entirely omits what is not unimportant. What then does '$\supset$' do ? It can hardly be said to state a connection : since often there is

nothing to be connected ; and, even where there are two things to be connected, there is never any third thing to connect them. Nor will the more cautious view, that it connects statements, quite do : since 'unasserted statement' contains a contradictio in adjecto.[1] But this question can be temporarily shelved. For meanwhile the more fully the relations between hypotheticals and the non-linguistic world are described, the less will semiotic labels matter.

Interpreted truth-functionally a general hypothetical is true or false in a very peculiar way. This peculiarity can be brought out by asking what a general hypothetical is about. If it is expandible into an endless conjunction of singular hypotheticals, clearly it is about what these are about. But these, like their parents, do not specify the situations to which they apply. η is related to the non-linguistic world in three alternative ways : it might be taken to apply either to this if it is a raven, or to this if it is not black, or to this tout court.[2]

Now it seems most natural to say that η and θ apply only where their antecedents are true (type 1 cases). Why? For it is at least an inevitable result of human myopia that θ cannot be completely verified, but only falsified (*a*). Why then is it unnatural to say that all situations which fail to falsify it confirm it equally ; and so that it applies both to things which are not black (type 2 cases), and to anything at all (type 3 cases) ?

First consider type 2 cases. Since η and its brothers can be contraposed, θ is equivalent to "If anything is not black it is not a raven." And, put like this, it applies to situations where the consequents of η and η's brothers are false. Why then do type 2 cases seem queer ? Because people are too myopic to verify general hypotheticals, and so want to make the fullest use of the limited evidence which they do get. They therefore consider that a general hypothetical does not merely escape falsification ; but is confirmed to the extent that it ran the risk of being falsified.[3] But it runs less risk of being falsified in a type 2 case than in a type 1 case. For, if there were a counter-example, it would be more likely to occur in a type 1 case than in a type 2 case : since the class of things which are ravens is smaller than the class of things which are not black (assumption A). This is an assumption about the classes picked out by descriptive

[1] I owe this point to an unpublished paper by Professor G. Ryle.

[2] See C. G. Hempel " Studies in the Logic of Confirmation " I, *Mind*, January 1945, particularly the footnote on p. 21 : II, *Mind*, April 1945 : C. H. Whiteley's reply to I, *Mind*, April 1945 : C. G. Hempel's rejoinder, *Mind*, January 1946 ; and Janina Hosiasson-Lindenbaum " On Confirmation ", *Journal of Symbolic Logic*, 1940, particularly pp. 136-141.

[3] See Janina Hosiasson-Lindenbaum, loc. cit., p. 134 footnote.

words, and so is beyond the purview of the logician. But the fact that people do make it explains why type 2 cases are thought to provide less confirmation than type 1 cases.

Next consider type 3 cases. Since η and its brothers can be transformed into disjunctions, θ is equivalent to "Anything is either not a raven or black". And, put like this, it applies to any situation whatsoever, whether the antecedents or consequents of η and η's brothers are true or false. Why then do type 3 cases seem queer, queerer even than type 2 cases? Again because people, being myopic, want graded confirmation. But θ runs less risk of being falsified in a type 3 case than it runs in a type 1 case, and even than it runs in a type 2 case. For, if there were a counter-example, it would be more likely to occur in a type 1 case than in a type 3 case, and more likely to occur even in a type 2 case than in a type 3 case: since the universe class is larger than the class of things which are ravens (assumption B), and larger even than the class of things which are not black (assumption C). These two assumptions too are about the classes picked out by descriptive words, and so are beyond the purview of the logician. But the fact that people do make them explains why type 3 cases are thought to provide less confirmation than type 1 cases and even than type 2 cases.

This shows why it is unnatural to attach the same importance to all three types of cases: and that the logician is professionally debarred from recognizing the greater importance of type 1 cases. And, since the oddities of '$\supset$' are the result of neglecting descriptive words, it is foolish to try to avoid them by seeking a stronger constant interpretation of 'If—then'. Anyway no other constant interpretation could be as closely related to the non-linguistic world as '$\supset$'. For instance, there could be no better evidence for the application of a binary logical predicate to two statements than there is for the use of the truth-functional connective '$\supset$'.[1]

It might now appear that type 2 cases and type 3 cases are utterly unimportant. For, if those three assumptions are made at all, it is natural to make them in such a strong form that type 2 cases and type 3 cases would provide a contemptible amount of confirmation. But, if the field of investigation were restricted to birds, an investigator would be safe if he tested θ by testing "(x) (bird x $\supset$ (raven x $\supset$ black x))" (γ). For, since

[1] W. V. Quine, "Mathematical Logic" 1947, p. 29, suggests that an appropriate binary logical predicate might be found to replace '$\supset$' in the interpretation of 'subjunctive' conditionals. Cf. R. Carnap "Logical Syntax of Language", pp. 254–5. I hope to show later that the problem of 'subjunctive' conditionals is part of the general problem of confirmation.

"(x) (raven x $\supset$ bird x)" (δ) is analytic, he would be sure that no counter-example of θ could fall outside this restricted field. Now type 2 cases and type 3 cases of θ would be thus parasitical on type 1 cases of γ. But still, within such limits, people do confirm general hypotheticals by examining type 2 cases and type 3 cases. Such experiments are rare, since even with this restriction it is obvious that type 2 cases usually,[1] and type 3 cases always provide less confirmation than type 1 cases. And, just because they are rare, the additional specification given by γ (which is not needed for type 1 cases of θ, since δ is analytic) is not usually expressed. But they do occur. Therefore type 2 cases and type 3 cases are not unimportant, and the notion that hypotheticals apply only where their antecedents are true is an error.

b. But all this still gives no criterion of reasonable general hypotheticals. For giving the highest common factor of the meaning of 'If—then' in general hypotheticals does not tell us when it is reasonable to utter them. For instance η and η's brothers are each said to mean negatively one thing, but positively any one of three things. And the reasonableness of θ might depend on how many of them mean which of these three things.

Now giving a criterion of reasonable general hypotheticals might be described as "giving the contextually varying extra meaning of 'If—then' in general hypotheticals"[2]. But whether this description is correct or not is a question which can be permanently shelved. For here too the relations between general hypotheticals and the non-linguistic world are more important than semiotic labels.

Certainly vacuous truth and copious truth are the two most obvious ways in which a general hypothetical might be (unfalsified but) unreasonable. And these two ways can be blocked by adding two more assumptions about the classes picked out by descriptive words : that there is at least one thing which is a raven (assumption D), and that there is at least one thing which is not black (assumption E). All five assumptions can now be compressed into the formula "$1 > \sim B > R > 0$" Now the importance of A, B and C is that they allow discrimination between the amounts of confirmation provided by cases of types 1, 2 and 3. But D and E play a more recessive rôle. For, if the speaker knew that D was false, he would know that

[1] Perhaps always. But see Janina Hosiasson-Lindenbaum, loc. cit., p. 140 footnote.
[2] See H. Reichenbach, "Elements of Symbolic Logic", p. 379, on a closely related question.

θ could never encounter a type 1 case, and so that it would always be vacuously true. And, if he knew that E was false, he would know that θ could never encounter a type 2 case, and so that it would always be copiously true. But the only point of his making assumption D is that, if he makes it, he assumes that θ has a chance of encountering a type 1 case. But, even if he does not make it, θ might still encounter a type 1 case for all he knows. And the only point of his making assumption E is that, if he makes it, he assumes that θ has a chance of encountering a type 2 case. But, even if he does not make it, θ might still encounter a type 2 case for all he knows.

General hypotheticals, taken as truth-functional statements, are like trawls which the speaker lets down into the non-linguistic world in order to catch a counter-example, a peculiar kind of fish. Now all the fish in the sea might be too long to fit into the trawl (perpetual copious truth: no type 2 cases). Or all the fish might be too thin to be caught by its mesh (perpetual vacuous truth: no type 1 cases). Or all the fish might be too long and too thin (perpetual copious and vacuous truth: no type 2 cases or type 1 cases, but only type 3 cases). But, unless the fisherman knew that all the fish were either too long (contradictory of E), or too thin (contradictory of D), or both, he could not know either that his trawl could never encounter a type 2 case, or that it could never encounter a type 1 case, or that it could never encounter either (respectively). Nor would he know that he could never catch a fish, and so that even type 3 cases would provide confirmation which simply was not needed: not needed, because, if he knew that fish were like that, trawling would be a work of supererogation. But, since he does not know this, he might catch a fish for all he knows. And, if he makes assumption A, he thinks that his trawl has more chance of catching when it approaches a fish which he knows is not too thin (type 1 case) than when it approaches a fish which he knows is not too long (type 2 case). And, if he makes assumption B, he thinks that his trawl has more chance of catching when it approaches a fish which he knows is not too thin (type 1 case) than when it approaches a fish which for all he knows might be too thin and too long (type 3 case). And, if he makes assumption C, he thinks that his trawl has more chance of catching when it approaches a fish which he knows is not too long (type 2 case) than when it approaches a fish which for all he knows might be too long and too thin (type 3 case).

But unfortunately vacuous and copious truth are not the only ways in which a general hypothetical can be unreasonable.

For nearly always D and E are not enough ; and the reasonableness of a general hypothetical depends on how many type 1 cases and how many type 2 cases it encounters. And sometimes D and E are too much ; and the reasonableness of a general hypothetical depends instead on its deducibility from another general hypothetical, or from other general hypotheticals, which themselves . . . etc. In fact the necessary condition of their reasonableness seems to be a complex disjunction which can be discovered only by careful investigation.[1] And, if this is true, the same careful investigation should discover the necessary condition of the reasonableness of singular counterfactual hypotheticals.[2] Anyway the notion that the necessary condition of reasonableness is single and simple, that the extra meaning of ' If—then ' is constant, is an example of a common error in philosophy, which might be called the ' Monistic Fallacy '. Here ends the discussion of *b*.

Before treating singular hypotheticals as statements it will be worth while to elaborate the close connection between the confirmation of general hypotheticals and their choice by elimination. (The connection is that speed of elimination, like strength of confirmation, is a function of the likelihood of finding a counter-example.) Now θ may be taken to say that *B* is a necessary condition of R, or that R is a sufficient condition of *B*, or both (however successful the elimination, ' necessary ' and ' sufficient ' remain always partly bluff).[3] And, if it were taken in the first way, it might be chosen by eliminating rivals which apply to the same type 1 cases. If so, situations where *R* was present would be examined, and a short list of strong candidates for the post of necessary condition of *R* would be chosen from among other properties which were present, say *B*, *C* and *D*. Then other situations where R was present would be examined, and in these other situations some of the three properties *B*, *C* and *D*, and some of their disjunctions [$B \vee C$], [$B \vee D$], [$C \vee D$] and [$B \vee C \vee D$] might be absent (conjunctive necessary conditions need not be considered)[4]. Thus this process of elimination might eventually leave *B* in the field. But, if it left *B* in the field, it would also leave all the disjunctions which included *B*, namely

[1] See H. Reichenbach, loc. cit., c. viii, for such an investigation of the reasonableness of general hypotheticals.

[2] See H. Reichenbach, loc. cit., c. viii also for such an investigation of the reasonablenes, of singular counterfactual hypotheticals : also R. M. Chisholm. " The Contrary-to-fact Conditional ", *Mind*, October 1946, and F. L. Will, " The Contrary-to-fact Conditional " *Mind*, July 1947.

[3] See C. S. Peirce, " Collected Papers ", Vol. II, p. 456 footnote : G. H. Von Wright, " The Logical Problem of Induction ", c. iv ; and C. D. Broad, " Hr. Von Wright on the Logic of Induction ", I, *Mind*, January 1944.

[4] See G. H. Von Wright, loc. cit., p. 73.

$[B \vee C]$, $[B \vee D]$ and $[B \vee C \vee D]$. And elimination could not enable us to choose between them. For, if any of these disjunctions were eliminated, B too would be eliminated. Therefore, it would be possible to choose between them only on other grounds. And elimination could leave a single candidate in the field only if that single candidate were the disjunction of the whole initial group $[B \vee C \vee D]$.

Symmetrically, if θ were taken in the second way, it might be chosen by eliminating rivals which apply to the same type 2 cases. It so, situations where B was absent would be examined, and a short list of strong candidates for the post of sufficient condition of B would be picked from other properties which were absent, say R, S and T. Then other situations where B was absent would be examined, and in these other situations some of the three properties R, S and T, and some of their conjunctions $[R.S]$, $[R.T]$, $[S.T]$ and $[R.S.T]$ might be present (disjunctive sufficient conditions need not be considered).[1] Thus this process of elimination might eventually leave R in the field. But, if it left R in the field, it would also leave all the conjunctions which included R, namely $[R.S]$, $[R.T]$ and $[R.S.T]$. And elimination could not enable us to choose between them. Therefore, it would be possible to choose between them only on other grounds. And elimination could leave a single candidate in the field only if that single candidate were the conjunction of the whole initial group $[R.S.T]$.

Of course either method might have failed to leave θ in the field. For B or R might have been eliminated. And $[B \vee C \vee D]$ or $[R.S.T]$ might have been eliminated. And B or R might not have been included in the initial short lists of strong candidates which are produced by a judgment of relevance (ravens croak too, and ebony too is black). But, if θ were eventually left in the field, then logically there is no reason to prefer the method which uses type 1 cases to the method which uses type 2 cases. For, only if assumption A is made, is there any reason to think that the method which uses type 1 cases will forward elimination more rapidly than the method which uses type 2 cases.

Finally, if θ is taken in the third way, it might be chosen by eliminating rivals which apply to the same type 3 cases. This method would treat all general hypotheticals as rivals, because they are all about the Whole of Reality. And, only if assumptions B and C are made, is there any reason to think that it will forward elimination less rapidly than the method which uses type 1 cases or the method which uses type 2 cases.

[1] See G. H. Von Wright, loc. cit, p. 73.

We must approach Nature not as pupils, but as masters; and as masters who pose questions which are likely to be answered quickly. And, when we choose general hypotheticals by elimination, we pose a disjunctive question "Is this one true, or that one, or . . . ?" and we pose it by observation and experiment. Now Nature never gives the answer 'Yes' to any limb of the disjunction, but only the answer 'No' to some or all of its limbs. But Nature is unlikely to give a quick answer 'No' to any limb of the disjunction unless the question is posed by a searching investigation. But, on assumption *A*, the investigation will not be sufficiently searching if it scrutinizes only situations where the consequents are false. And, on assumptions *B* and *C*, it will be even less searching if it relies merely on an indiscriminate rummaging of the whole universe.

This parenthesis may be closed with the observation that γ plays the same role in θ's choice by elimination that it played in θ's confirmation.[1]

The next three difficulties, *c*, *d* and *e*, are presented by singular hypotheticals taken as truth-functional statements.

c was that the verification of singular counterfactual hypotheticals is always peculiar (not 'subjunctive hypotheticals', since the falsehood of the antecedent in modern English is usually conveyed not by mood but by tense and auxiliaries). For disagreement about the truth-value of a singular counterfactual hypothetical is compatible with agreement about the truth-values of its components. Its verification does not exhaust the speaker's full meaning: he also means that this *would have been* black if it *had been* a raven. Yet this extra meaning, if supportable, is borrowed from the parent general hypothetical, and so the evidence is truant from the single situation. Nor is this truancy surprising. For the singular counterfactual hypothetical would hardly have been uttered unless it had been deduced from its well confirmed parent general hypothetical. And again there is little point in debating whether the semiotic label 'meaning' is to be restricted to the highest common factor of meaning supported by the evidence in the single situation; or is to be extended so as to include the varying extra meaning supported by the truant evidence. It is more important to see exactly how singular counterfactual hypotheticals are related to the non-linguistic world; that their extra meaning, if it is concentrated on the single situation, cannot be

[1] Professor G. H. Von Wright, "Confirmation", *Xth International Congress of Philosophy*, Fasc. II, p. 794, seems to argue against all that I have said about the queer ways of confirming and eliminating general hypotheticals. But this is only because he does not there consider type 3 cases at all, or type 2 cases in relation to sufficient conditions.

evidential, but only 'picture preference'.[1] For we may prefer to picture a connection indicated by each singular hypothetical (an idle connection if the singular hypothetical is counterfactual); but the evidential support of this extra meaning can only be truant.

For instance, this peculiarity in the way singular counterfactual hypotheticals are related to the non-linguistic world might be described by saying that they are interpretations.[2] (Another reason for saying this will be given later). Or it might be described in more detail by saying that their verification does not exhaust their full meaning: that complete evidence for them would include the (unattainable) complete evidence for their parent general hypotheticals; and that their verification yields little confirmation for their parent general hypotheticals, because it is not got from type 1 cases, and so, by reflection, little support for their own extra meaning. But it must not be described by saying that they are really about hypothetical facts. For this description looks like an answer to the question " How are they related to the non-linguistic world ? " but what it actually says is that they are related to a world which is neither linguistic nor non-linguistic. And an alleged close link with a logically inaccessible world is no substitute or consolation for a familiar loose link with the sublunary world. *c* is true but inevitable.

Now the second, more detailed, description has the advantage of revealing that *c* is only a special case of the general difficulty *b*. For a singular counterfactual hypothetical yields little confirmation for its parent general hypothetical only because it cannot apply to a type 1 case. It can be verified only either in a situation which endorses its components as false-false or in a situation which endorses its components as false-true (in an *FF* situation or in an *FT* situation). But an *FT* situation yields even less confirmation than an *FF* situation. For a type 1 case is a situation which could develop into *TF*: and, whereas an *FF* situation could develop out of a type 2 case, an *FT* situation could develop only out of a type 3 case. In fact an *FT* situation is unique in this respect. For an *FF* situation could develop out of either a type 3 case or a type 2 case or both: and a *TT* situation could develop out of either a type 3 case or a type 1 case or both. Nor is it surprising that there is an option about the way situations *FF* and *TT* can be taken. For amount of confirmation depends on probability of falsification,

[1] See John Wisdom, " Other Minds ", I, particularly p. 381, *Mind*, October 1940.
[2] See Stuart Hampshire, " Subjunctive Conditionals ", ANALYSIS, October 1948.

which is relative to evidence contained in a case. So, both when a singular hypothetical is verified in an *FF* situation and when it is verified in a *TT* situation, there are two alternative possible claims to the amount of confirmation which it yields for its parent general hypothetical. But *c* was described by saying that singular counterfactual hypotheticals yield little confirmation for their parent general hypotheticals. Therefore *c* is only a special case of the general difficulty *b*, which has already been discussed. Therefore it is partly an error to say that singular counterfactual hypotheticals present a peculiar difficulty (only partly an error for reasons which will be given in the third part of this paper).

It is now possible to show that *d* too is only another aspect of the general difficulty *b* which has already been discussed, that the paradoxes are not really paradoxical. For an *FF* situation is associated with the paradox of trivial truth: a *TT* situation with the paradox of ponderous truth; and an *FT* situation with both paradoxes. Thus trivial truth is incompatible with a type 1 case: ponderous truth with a type 2 case; and both with either a type 1 case or a type 2 case. But, if we concentrate on the situation in which a singular hypothetical is verified and refrain from stealing surreptitious glances at the truant evidence, then these paradoxes cease to be paradoxical. For, when there is no type 1 case, the property mentioned in the antecedent is absent: and any property, present or absent, might be the necessary condition of an absent property. And, when there is no type 2 case, the property mentioned in the consequent is present: and any property, present or absent, might be the sufficient condition of a present property. And, when there is no type 1 case or type 2 case, both these difficulties are combined. Now it is true that we do not use ' follows from ' like this. But that is not because the situations in which singular hypotheticals are verified exhibit some connection: but only because we do not utter singular hypotheticals unless they have been deduced from parent general hypotheticals which have been confirmed already to some extent.

But singular counterfactual hypotheticals do not appear to be deducible from their parent general hypotheticals[1] (difficulty *e*). However this appearance is partly illusory and partly presents no difficulty. For it is true that a singular counterfactual hypothetical implies that its antecedent is false, and that the falsehood of its antecedent cannot be deduced from its

[1] See William Kneale, " Probability and Induction ", p. 75: and *Proceedings of the Aristotelian Society*, Supplementary Volume XXII, pp. 163–5.

parent general hypothetical. But this presents no difficulty. On the other hand the impossibility of deducing its purely hypothetical element (interpreted truth-functionally) from its parent general hypothetical, which would have been a real difficulty, is in fact an illusion. It is an illusion produced by concealing the universal reference of the parent general hypothetical.[1] The truth is rather that the parent general hypothetical is not completely verifiable: that, if its confirmation had already involved the verification of the singular counterfactual hypothetical, the deduction would be otiose; and that a singular counterfactual hypothetical often has more than one parent. This last complication is an additional reason for calling singular counterfactual hypotheticals 'interpretations'.[2] For, if a singular counterfactual hypothetical stands at the point of intersection of several general hypotheticals, judgment is needed in order to assess what would have been the outcome (particularly in history). But then each aspect of the singular counterfactual hypothetical is still deducible from its own parent general hypothetical, and judgment is needed only in order to conflate the results of the separate deductions.

It might still be objected that it is impossible to deduce what *would have been* the case from what *is* (timelessly) the case.[3] But this is tantamount to the old objection that singular counterfactual hypotheticals cannot be interpreted truth-functionally: about which enough has been said. After all, from what other confirmable sentence could a singular counterfactual hypothetical be deduced? From a sentence indicating a connection? But such a sentence expresses only 'picture preference'. Or from a sentence describing hypothetical facts? But, if, because the actual world houses the situation *FF*, a shadow-world is postulated in order to house the situation *TT*, parity of reasoning demands two more shadow-worlds, one for the situation *FT* (since singular counterfactual hypotheticals do not present a peculiar difficulty), and one for the situation *TF* (for 'negative facts'). And, even if we dropped 'negative facts', each singular hypothetical (of whatever kind) would still require three worlds, one actual and two possible; and so each general hypothetical would require an infinite number of worlds, produced by combining in various ways the three worlds of each member of its infinite family; only one of which would be actual. Hypothetical facts are the objects of an insatiable craving. From what

[1] See K. R. Popper, "A Note on Natural Laws and so-called 'Contrary-to-fact' Conditionals", *Mind*, January 1949.
[2] See Stuart Hampshire, loc. cit.
[3] See William Kneale, *loc. cit.*

other confirmable sentence then could a singular counterfactual hypothetical be deduced ?

Here, at the end of the first part of this paper, it might seem appropriate to reopen the shelved question " If hypotheticals are statements, exactly what sort of statements are they ? "[1] But, since the reasons for affixing semiotic labels are more important than the labels themselves, it is no longer imperative to answer it.

But nothing has yet been said about the reasons for calling hypotheticals 'rules'. And this second part of this paper is the place to give them. However, it is beyond its scope to give them fully.[2] Briefly, they may be called rules in so far as they guide the user in inferring the unknown from the known (strictly nothing is known in a type 3 case) : and they may be called rules because they are not only applied, but also learnt, possessed and taught. And a general hypothetical, which is an inexhaustible sheaf of singular hypotheticals, may be regarded as a rule which can be applied many times, just because it has many clauses, each of which can be applied only once.

But this way of looking at hypotheticals does not evade the difficulties *a—e*. For, when a singular hypothetical is taken as a truth-functional statement, it is shown to be true by the same situation which shows it to be correct (and so of no further use) when it is taken as an applied rule. And, when a general hypothetical is taken as a truth-functional statement, its truth is confirmed by the same situations which confirm its correctness (and so its uselessness henceforward in these situations) when it is taken as an applied rule. In fact the only effect of regarding hypotheticals as applied rules rather than as truth-functional statements is to substitute 'correct' for 'true' in the meta-language.[3]

However, if hypotheticals are regarded as rules, the difficulties *a—e* do become less urgent. There is of course a sense in which the speaker (or hearer or silent possessor) always has his eye open for a possible counter-example to a general hypothetical, whether taken as a statement or as a rule—or anyway not closed to a possible counter-example : and in which he eventually applies a singular hypothetical, whether taken as a statement or as a rule, even when the situation is FT. And in this sense general and singular hypotheticals apply not only to type 1 cases and

[1] See Lewis Carroll, " A Logical Paradox ", *Mind*, July 1894.

[2] See F. P. Ramsay, " General Propositions and Causality " in " *The Foundations of Mathematics* ".

[3] See I. Berlin, *Proceedings of the Aristotelian Society*, Supplementary Volume XVI p. 66, for a closely related argument.

type 2 cases (after contraposition), but also to type 3 cases ("$\sim R \vee B$"). But the speaker teaches, the hearer learns and the silent knower possesses a rule which is, in another sense, in abeyance, usually until a type 2 case or, better still, a type 1 case appears. And these two senses can be marked by distinguishing rules in application from rules in abeyance. And, because rules often are in abeyance, the difficulties *a—e* become less urgent when hypotheticals are taken as rules.

Finally, there is a complication, neglect of which leads to error. Hypotheticals, when applied, can always be regarded as statements, but not always as rules. For an applied rule is used in order to guide an inference from the known to the unknown. But, for instance, a singular counterfactual hypothetical cannot be used in order to guide an inference from the known to the unknown. And this is not only because, since the antecedent is known to be false, it cannot be applied to a type 1 case: but also because, although the consequent is known to be false, it cannot be used in a type 2 case, since the truth-value of the antecedent is known too; and also because, since the truth-values of both antecedent and consequent are known, it cannot be used in a type 3 case either. Hence singular counterfactual hypotheticals cannot be regarded as applied rules. They are obsolete clauses of rules, obsolete even at the moment of their origin. But hypotheticals expressed by "If—then" cry out for application to a type 1 case: type 1 cases are more important; and their greater importance is reflected by the alternative locutions which put the antecedent as a question or as a command. And this strong suggestion produces the erroneous notion that either there is a shadow-world of hypothetical facts in which singular counter-factual hypotheticals will be able to apply to type 1 cases; or else, since there is no actual situation to which they apply as rules, therefore there is no actual situation to which they apply as statements.

I have gradually slipped into talking about 'general hypotheticals' instead of about 'general hypotheticals like θ', and about 'singular hypotheticals' instead of about 'singular hypotheticals like η'. But are all general hypotheticals like θ, and all singular hypotheticals like η or η's counterfactual brothers? Clearly not. For instance "If he was surprised he didn't show it" is unlike η for three reasons. Its components are not both of the subject-predicate form: 'If' is used concessively; and, if 'it' means genuine surprise, the class of situations which endorse the consequents of its brother singular hypotheticals as false cannot be larger than the class of situations

which endorse their antecedents as true. But what, in full detail, makes hypotheticals too unlike η and θ to be treated like them ?

To answer this question is beyond the scope of this paper. Instead this third part of it will be devoted to showing how singular hypotheticals can be embedded in different locutions. This will not be done for general hypotheticals : partly because the locutions in which they are embedded do not vary in such an interesting way, and partly because a comparison of the different locutions in which the purely hypothetical element of singular hypotheticals can be embedded will throw some light on the position of singular counterfactual hypotheticals. These locutions are set out in the following table (in which LO = leaves the truth-value open : IT = implies that the truth-value is 'true' ; and IF = implies that the truth-value is 'false').

	Antecedent	Consequent	Locution
i	I T	I F	(None used by sane people).
ii	I T	I T	"This is black because it is a raven."
iii	I F	I F	"If this had been a raven it would have been black."
iv	I F	I T	"If this had been a raven it would have been a reason for the fact that it is black."
v	I T	L O	"Since this is a raven, it must be black." (Here 'must', though strong, is bluff, and leaves the truth-value of the consequent open).
vi	I F	L O	"If this had been a raven it would have been a reason for its being black."
vii	L O	I T	"If this is a raven, it is a reason for the fact that it is black."
viii	L O	I F	"If this had been black its being a raven would have been a reason."
ix	L O	L O	"If this is a raven it is black." "If this is not black it is not a raven." "Either this is not a raven or it is black." (These are the three ways of expressing a pure hypothetical).

Now this table is not complete on its right-hand side. Nor does it say all that might be said about the locutions which it does list on its right-hand side : it merely classifies them according to the exhaustive division given on its left-hand side. But it does throw some light on the position of singular counterfactual hypotheticals (iii). For iii, like all the other locutions except ix, is not a pure hypothetical, but contains a purely

hypothetical element. Also it shares inapplicability to type 1 cases only with iv and vi. And type 1 cases are the most important. And iv and vi are far rarer than iii. Hence iii holds a unique position. But ii, iv and vii are alone inapplicable to type 2 cases. And, since iv and vii are far rarer than ii, ii also holds a unique position, complementary to the unique position held by iii. And iv too holds a unique position which combines the disadvantages of the other two, being the only locution—and a rare one—which is not applicable either to type 1 or type 2 cases. Now ii, iii and iv present the same difficulty in different forms. ii and iii present it more insistently than iv, because they are common while it is rare. And iii presents it more strikingly than ii, because, unlike ii, it is expressed by 'If—then'. But, when it is seen that no locution can apply to more than two types of cases, and that for all three locutions the difficulty arises from the relative importance of the three types of cases, it is clear that it is the inevitable result of the logician's professional neglect of descriptive words. And, when the three assumptions which discriminate the amounts of confirmation provided by the three types of cases are made explicit, the difficulty is largely dissolved. There still remains the residual difficulty that, for instance, locutions which logically cannot apply to type 1 cases causally (or logically, given A and B) cannot yield as much confirmation as those which can. But this is inevitable from any point of view. One can only explain and acquiesce.

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SOME COMMENTS ON TRUTH AND DESIGNATION

By R. M. Martin

IN two recent papers, Messrs. Max Black[1] and P. T. Geach[2] have discussed problems connected with designation and truth. Mr. Black's paper follows in essential respects the classic analysis of these concepts due to Tarski,[3] and reiterates in particular the point made by him that the semantic conception of truth is not apparently applicable to non-formal, " common

[1] Max Black, " The semantic definition of truth ", Analysis, 8, 4, 1948, pp. 49–63.
[2] P. T. Geach, " Designation and truth ", Analysis, 8, 6, 1948, pp. 93–96.
[3] A. Tarski, " Der Wahrheitsbegriff in den formalisierten Sprachen ", *Studia Philosophica*, 1, 1935, pp. 261–405. This paper will be referred to as WFS. See also Tarski's " The semantic conception of truth ", *Philosophy and Phenomenological Research*, 4, 1944, pp. 341–375.

sense " languages. Mr. Geach points out an important flaw in one of Mr. Black's statements and attempts to correct it. In this present note, several comments and criticisms are offered concerning these papers, particularly of Mr. Geach's.

Mr. Geach's criticism of Black appears to be essentially correct. He points out that Black's statement :

(A) For all x and y, if x is a sentence and y uniquely designates x, then y is true if and only if x.[1]
is meaningless.[2] Black is here, in fact, confusing what are now ordinarily called, following Quine, the *use* and *mention* of expressions.

Mr. Geach proposes to " correct " the statement (A), and finds that the results of so doing are " rather surprising ". Instead of doing this, however, he offers what is apparently intended as an alternative definition of the semantical predicate ' true '. Let us first note Mr. Black's own improved version of (A), and then consider Mr. Geach's proposed definition of ' true '.

Mr. Black rejects (A) as a definition of ' true ' (for a suitable language L). What it is intended to provide is presumably a statement of the conditions under which a definition of ' true ' is said to be, following Tarski, *materially adequate*. Mr. Black rephrases this condition as follows. Consider the schema

(S) $$s \text{ is true} \equiv x.$$

Of this Mr. Black says, " We may say, informally and inexactly, that an acceptable definition of ' true ' must be such that every sentence obtained from S by replacing ' x ' by an object sentence and ' s ' by a name or definite description of that object sentence shall be true."[3] As far as the use and mention of expressions are concerned, this statement is unobjectionable. Thus, although Mr. Black intends (A) to be a meaningful statement when in fact it is not, he himself provides immediately following it, an unobjectionable formulation of what (A) was possibly intended to state.

There is, however, an important respect in which Mr. Black's statement of the condition of material adequacy (quoted above) is inaccurate, even allowing for the informality of the discussion. It is not merely that all statements of the metalanguage of the form S, satisfying the given requirements, should be *true*, but rather that all such statements must be *provable* within the metalanguage utilizing the given definition of ' true ', This is by no means a trivial point if Tarski's results are to be stated

[1] Black, p. 51. [2] Geach, p. 93. [3] Black, p. 52.

and discussed carefully. The important differences between the concepts of truth (in a given formalism) and provability (in that formalism) are too well known to need elaboration here.[1]

Let us now turn to Mr. Geach's conception of truth. He let's ' DES(L) . . . ' stand as an abbreviation for the phrase ' that which has in the language L the designation . . . '. He then offers the formula.

(E) For all x, x is true in L $\equiv$ DES(L) x.[2]

This is intended to provide a definition of ' true in L ' in terms of ' designation in L '.

Mr. Geach does not specify in any way the character of the languages to which his definition is to be applicable. From some of his examples it is clear that he intends it to provide for the concept of truth as used in everyday speech. Now it is clear from the work of Tarski that the language of everyday speech is ' semantically closed ' and hence inconsistent.[3] Mr. Geach's definition should thus at best be regarded as providing for a semantical concept of truth as applied to languages whose formal structure is carefully specified. So regarded, however, there are several important objections to be urged against it.

For one thing, Mr. Geach nowhere analyzes or defines the concept of designation which he uses. If he intends it as an undefined concept of semantics, the axioms characterizing it should be fully specified. But even if this were done his procedure would still be less satisfactory than that of Tarski. It is a merit of Tarski's method that a designation relation is wholly definable within the framework of the concepts he admits. This is also true of the relation of satisfaction in terms of which the definition of ' true in L ' is couched.[4] Lacking a precise characterization or definition of ' DES(L) . . . ' Mr. Geach cannot be regarded as having provided a definition of ' true in L ' at all.

Let us attempt to formulate the kind of definition Mr. Geach is apparently seeking, following as closely as we can the hints he gives us. Let ' x ' be a variable ranging over the symbols of finite sequences (concatenates) of symbols of L. In particular, then, ' x ' can range over the sequences of symbols of L called *sentences* (well-formed formulae). In addition to having variables range over sentences, Mr. Geach also " assumes the legitimacy of talking about what *sentences* designate, as well as what *names* designate ".[5] In other words, Mr. Geach wishes to admit metalinguistical variables ranging over what sentences desig-

[1] See especially Satz 7, p. 318, of WFS. [2] Geach, p. 94. [3] See p. 275 of WFS.
[4] See Definition 22, o. 311, of WFS. [5] Geach, p. 94.

nate. Let 'p' and 'q' be variables ranging over such entities. (Presumably these entities are not intended to be truth-values. Otherwise it is not clear that Mr. Geach would be giving a *semantical* definition of 'true in L').

A natural meaning for 'DES(L) x' would appear to be *the one entity designated in* L *by* x. (Presumably Mr. Geach subscribes to what, following Carnap, may be called the *Principle of Univocality*,[1] which states roughly that a symbol can name one and only one object). An analysis of phrases of the kind 'the one entity of which such and such holds' is provided by the theory of descriptions. Hence, 'DES(L) x' might be thought of as definiendum for '(ιp) (DES(L) x, p)' as definiens, where the scope of '(ιp)' is read 'x designates in L the entity p'. (We presuppose a designation relation as primitive). If this interpretation of Mr. Geach's intentions is correct (see below), (E) becomes via description theory (see *14.01 of *Principia Mathematica*).

(F) $(x)\,(\exists q)\,((p)\,(\text{DES(L)}\,x, p. \equiv . p = q).(x \text{ is true in L} \equiv q))$.

This formula is of course meaningless, unless phrases of the kind '$p = q$' have been properly defined. The problem of characterizing identity between entities which sentences designate, barring truth values, is as is well-known, enormously difficult. Lacking a characterization of such identity, and assuming that the equivalence of (E) and (F) does justice to Mr. Geach's intentions. (E) cannot then be regarded as being a definition at all.

Mr. Geach is an intensionalist in the sense of being willing to admit variables of the kind 'p' and 'q' as well as quantification upon them. Intensional definitions of 'true' of the kind Mr. Geach is perhaps seeking have already been provided by Carnap, in the very passages to which Mr. Geach makes reference.[2]

The meaningless formula (F) would appear to provide a reasonable interpretation of Mr. Geach's views. In it 'DES(L)' figures as a relational term. For obscure reasons, however, Mr. Geach does not regard this as a relational term. Because 'DES (English) 'Chicago is a large city'' is a "complete sentence", Mr. Geach says that 'DES (English)' is not a relational sign. Presumably also 'Des(L)' is not to be taken as a relational sign. Mr. Geach nowhere tells us what kind of a sign it is intended to be. Because 'DES(L)' is not a relational sign, Mr. Geach states that he has eliminated problems about 'the designata of

[1] R. Carnap. *Meaning and Necessity*, University of Chicago Press, 1947, p. 98.
[2] R. Carnap, *Introduction to Semantics* Harvard University Press, 1942, pp. 49–55.

sentences' and about 'the relation of designation'. It would seem to be clear from the remarks above that, rather than having eliminated any problems, Mr. Geach has not even roughly, let alone with the necessary care and precision, described the concepts in terms of which he proposes the elimination to be carried out.

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SOME CONSEQUENCES OF PROFESSOR A. J. AYER'S VERIFICATION PRINCIPLE

By D. J. O'Connor

I

ON page 13 of the Introduction to the new edition of *Language, Truth and Logic*, Professor Ayer proposes an emended version of his verification principle in the following form:

> I propose to say that a statement is directly verifiable if it is either itself an observation-statement, or is such that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone; and I propose to say that a statement is indirectly verifiable if it satisfies the following conditions: first, that in conjunction with certain other premises it entails one or more directly verifiable statements which are not deducible from these other premises alone; and secondly, that these other premises do not include any statement that is not either analytic or directly verifiable or capable of being independently established as indirectly verifiable. And I can now reformulate the principle of verification as requiring of a literally meaningful statement which is not analytic that it should be either directly or indirectly verifiable in the foregoing sense.

He found that this reformulation of the principle was called for because his previous formulation had been too liberal and admitted any statement whatsoever as meaningful including such indisputable nonsense as "The Absolute is lazy". I want to show (1) that even in its new form the principle is still too liberal and is, in fact, no more stringent than before; and (2) that it has certain other defects.

Ayer does not give any examples to illustrate the application of his new criterion but it is not too difficult to provide some simple illustrations. Clearly, any conditional statement whose protasis and apodosis are both observation-statements will satisfy the criterion of direct verifiability. (An observation-statement is defined on page 11 as a statement " which records an actual or possible observation ".) And it does not matter whether the conditional in question is contrary-to-fact or not. Consider the following examples :

A1. If it had frozen last night, the pipes would have burst.

B1. If Dewey had been President in 1949, there would have been no earthquake in Seattle in April.

C1. If Jones were dead, he would be happy.

A1, B1 and C1 all satisfy Ayer's criterion for direct verifiability, for they respectively entail, in conjunction with the observation-statements

A2. The pipes did not burst.

B2. There was an earthquake in Seattle in April.

C2. Jones is not happy.

the further observation-statements

A3. It did not freeze last night.

B3. Dewey was not President in 1949.

C3. Jones is not dead.

And no one of A1, B1 or C1 can be deduced from the other premises taken singly.

Now it is difficult to accept the conclusion that A1, B1 and C1 are all, in the same sense, directly verifiable. Commonsense would regard A1 as more susceptible of verification than B1 because there is plenty of positive evidence which is relevant to A1 but none which is relevant to B1. Moreover C1 would normally be said to be unverifiable even if not metaphysical in Ayer's pejorative sense of the word. The procedure for verifying these and other conditionals presumably consists in using them as major premises to prove observation-statements which can then receive independent observational verification. If the observation-statement used as the minor premise is verified and the conclusion falsified, by observation, the hypothetical will thereby be proved false. We cannot, of course, prove it true by this means.

Thus it will be seen that Ayer's principle not only makes conditionals, including contrary-to-fact conditionals, verifiable but ostensibly provides a procedure for determining their falsity in certain cases. However, it is not a useful procedure as where it can be applied it merely amounts to showing by

observation that the apodosis is false and the protasis true. But we already know, on other grounds that " if p, then q " is false when p is true and q is false and so could falsify a conditional by direct observation without appeal to other premises or to Ayer's principle, whenever the protasis is observably true and the apodosis observably false. Moreover, the procedure is not applicable to the case of contrary-to-fact conditionals, where, by definition, the protases are false. The chief failing of the principle in this respect is that it places certain statements into the class of verifiable propositions without providing any procedure for determining what their truth-values are. And how can it be meaningful to say *that* a statement is verifiable if we do not know, even in principle, *how* to determine its truth-value? This is precisely the metaphysicians' error from which verification principles are designed to deliver us.

And if it should be objected that A3, B3 and C3 can be deduced *directly* from A1, B1 and C1 respectively on the ground that contrary-to-fact conditionals are properly expressed in the form " if p, then q; but not-p ", then it follows, of course, by Ayer's principle that such conditionals are not verifiable, directly or indirectly, and so are meaningless. But this conclusion is surely even more paradoxical than the suggestion that they are verifiable even though we do not know, even in principle, how to verify them.

II

But there are far more serious consequences of the principle to be considered. For a conditional statement to qualify as " directly verifiable " it is sometimes sufficient for the protasis or apodosis only to *contain* an observation-statement as a component; it is not necessary that it should consist entirely of one or more observation-statements. The conjunction of A1 and A2 to prove A3 is an argument of the form: if p, then q; but not-q; therefore not-p. But we can prove not-p equally well by the valid form: if p, then q and r; but not-q; therefore not-p. We can say, in other words, that the following statement is directly verifiable:

D1. If this tiger is vegetarian, then he is not dangerous and the Absolute is lazy.

because D1 entails, in conjunction with the observation-statement

D2. This tiger is dangerous

the further observation-statement

D3. This tiger is not vegetarian.

Moreover an argument whose major premise is an hypothetical with a disjunctive protasis will provide equally awkward examples, for instance :

E1. If either the Absolute is lazy or this tiger is dangerous then this tiger is not vegetarian.
together with the observation-statement

D2. This tiger is dangerous
entails the further observation-statement

D3. This tiger is not vegetarian.

D1 and E1 seem to me to constitute a refutation of the suggestion that Ayer's revised principle of verification can be used to discriminate meaningful from non-meaningful statements. For it surely must be admitted that no molecular statement can be directly verifiable unless all its components, other than analytic statements, are so as well. And since we cannot admit that the statement "The Absolute is lazy" is directly verifiable, the principle, in so far as it relates to direct verifiability, will have to be further restricted. I suggest the following reformulation :

> A statement is directly verifiable if it is (1) either itself an observation-statement or satisfies the following conditions : first, that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone ; and (2), that if it is a molecular statement, it should not contain any components which are not either analytic or themselves observation-statements.

III

The fact that the definition of direct verifiability is defective will, of course, render the definition of indirect verifiability defective also. But even if the former definition is suitably emended the latter requires further adjustment.

In the first place, as it stands, the definition is too wide and covers direct as well as indirect verifiability. Thus, on this definition, a statement can very well be both directly and indirectly verifiable. For example, A1 falls into both categories because A2 and A3 are directly verifiable *in virtue of being observation-statements*. Moreover, a simple observation-statement has to be classed as *indirectly* verifiable on the definition given. D2, for example, in conjunction with :

F1. If this tiger is vegetarian, then he is not dangerous
yields D3 which is directly verifiable. D2 is therefore indirectly verifiable.

But the same difficulty arises in another way which may be illustrated by a simple example

G1. All arachnids have eight legs.

is directly verifiable because in conjunction with an observation-statement

G2. This is an arachnid

it entails the further observation-statement

G3. This has eight legs.

On the other hand, G1 in conjunction with a directly verifiable statement which is not an observation-statement

H1. All spiders are arachnids

yields another statement

H2. All spiders have eight legs

which is of the same type as H1. G1 therefore by the second part of the verification principle is an *indirectly* verifiable statement also.

In other words, Ayer has made the level of verifiability of a statement, other than an observation-statement, dependent on the nature of the statements to which it stands in certain logical relations. And as the same statement may stand in the specified logical relations to both observation-statements and to other verifiable statements there seems to be no reason why it should not be at once directly and indirectly verifiable. Perhaps it may be considered unobjectionable that one statement should fall into two categories in this way. However I do not suppose that the consequence was anticipated when the principle was framed. In any case, it is clearly desirable to remove this imprecision. The difficulty, if it is one, can be remedied by adding a further condition to the definition of indirect verifiability as follows : " thirdly, that the statement in question should not itself be directly verifiable ".

And finally, we shall, of course, have to add another condition analogous to the one we added to the definition of direct verifiability, in order to avoid admitting as indirectly verifiable statements like :

J1. If all tigers are carnivorous or the Absolute is lazy, then all tigers are dangerous.

The verification principle in the emended form I have suggested will then read as follows :

> A statement is directly verifiable if it is (1) either itself an observation-statement or (2) satisfies the following conditions: first, that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone ; and

secondly, that if it is a molecular statement it should not contain any components which are not either analytic or themselves observation-statements.

A statement is indirectly verifiable if it satisfies the following conditions (1), that in conjunction with certain other statements it entails one or more directly verifiable statements which are not deducible from these other premises alone; (2), that these other premises do not contain any statement which is not either analytic or directly verifiable or capable of being independently established as indirectly verifiable; (3), that the statement should not itself be directly verifiable; and (4), that if it is a molecular statement, it should not contain any components which are not either analytic or directly verifiable or capable of being independently established as indirectly verifiable.

But I do not feel at all certain that this principle, cautiously complicated though it is, could not be shown to admit some unacceptable cases to the status of meaningful statements. But this could be shown only by the construction of a suitably varied selection of concrete examples. Even in its revised form, it allows meaning to C1 and to a wide range of contrary-to-fact conditionals. And it does this without suggesting a procedure for carrying out the verification. Might it not be better to abandon this rather patched and leaky vessel and construct a new one?

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